ADDENDUM II

In this space the author makes a critical response to the statements and opinions of several colleagues, representatives of ornithological committees and forums on a national and global scale, who colourfully elaborated on the validity of three species recently described and published in different editions of this Audiornis guide. This response is in honour of the users of the Audiornis guides, who are owed an explanation as to why such taxa new to science are denied in other jurisdictions, but continue to be included in our books and various sites (including international jurisdictions) as true species.

The closing date for the edition of this book, and its respective addenda, was 30 December 2022. The following observation was added when the galley proof as corrected:

“The publication of ‘Monteleone and Pagano (2022) *Listado de las Aves Argentinas, Temas de Naturaleza y Conservación Nº 12*, Aves Argentinas/AOP’; so long awaited by our editorial team during the year 2022, was only released to the public on 5 January 2023 (M. Masariche pers. com.). This explains why there is no mention of this important study in this guide, even though its publication date is listed as 2022."

AUDIORNIS

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On the taxonomic status of three recently described Passeriformes: *Sporophila digiacomoorum* (López-Lanús, 2015), *Sicalis holmbergi* (López-Lanús, 2017) and *Cinclodes lopezlanusorum* (López-Lanús, 2019); 'Putting the bell on the cat' and the political bias of ornithology.

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Introduction

I am the author of the nomination of three bird species new to science described since 2015, which are the Iberá Seedeater (*Sporophila digiacomoorum*, López-Lanús 2015b), Ventania Yellow-Finch (*Sicalis holmbergi*, López-Lanús 2017), and the Lenga Cinclodes (*Cinclodes lopezlanusorum*, López-Lanús 2019). All three nominations have been challenged and rejected by renowned ornithologists who represent, or are associated with, prestigious academic or non-academic institutions with a long history in the study of birds. Until now, after seven years, I have only responded with silence to so many arguments published by various institutions (ornithological records committees, etc.), some publications ('scientific' articles) and a copious amount of informal comments (on social media). Enough time to 'hear it all". The errors in these sources are so implausible and versatile that it is time for me to speak out and thus reverse the well-known philosophical principle of 'he who is silent, gives'.

Results

Sporophila digiacomoorum (López-Lanús, 2015): **IBERA SEEDEATER**.

"My brother does not believe that your species is new to science, he told me directly that he is never going to respond to the changes requested by the Auk reviewers" (A.G. Di Giacomo, pers. com. MAR/2015). With this sentence I understood the futility of my effort to get A.S. Di Giacomo as senior author to reply to the editor of the well known journal of the American Ornithologists Union, the requested amendments to our 2012 article. In López-Lanús (2015) this situation is mentioned in a new paper, describing why the previous paper was abandoned, and also the need for the description because it is indeed a new species. It reads as follows (an excerpt translated from Spanish): *"This addendum is justified because a new species of* Sporophila *for science, discovered* [by BLL] *in 2010 on the basis of its vocalization, remains undescribed to date (December 2015).. / …With Adrián Di Giacomo as first author and Cecilia Kopuchian, we prepared a manuscript describing the species… / It was submitted to a well-known ornithological journal in 2012. Shortly afterwards its editor approved the article for publication subject to some editorial amendments. However, inexplicably, the senior author and co-author* [C. Kopuchian] *avoided making the requested changes by allowing time* [three years] *to pass for various reasons: all technically valid, but easy to overcome. At the end of*

2015 and in view of the fact that the publication of the species was abandoned despite my multiple efforts; added to the fact that I interpret that my co-authors prefer not to be professionally engaged in publishing a species on the basis of its bioacoustic analysis (the 'capuchin' group being complex to study), due to their strictly academic position, I assume the duty to describe it as their mentor." For a better understanding go to the source: López-Lanús (2015b).

The ISBN (International Standard Book Number) for the guide by López-Lanús (2015b) is 978-987-33-9633-5 with publication date December 2015 (see: ISBN 2015/2022). At the time of summarising the contents of the book as requested by the ISBN, it was added (an excerpt translated from Spanish): "*Supplement / Final addendum with description and nomination of a bird new to science nominated* Sporophila digiacomoi [sic] *by the author, the same bird informally mentioned in the publication of López-Lanús et al. 2013 (pp.209-211) without a scientific name*". This information has been public since that date. The type of sales option of the guide states: "*Product produced on demand (Printed on demand)*".

All this clarification is necessary because Claramount and Areta (2016) base their exposition on discarding the physical existence of López-Lanús (2015b) on the date they published. They treated the description of *Sporophila digiacomoorum* as coming from a digital version, a preprint to formal publication, which does not meet the requirements of the International Code of Zoological Nomenclature (ICZN). They emphasised: "*…The new species is described as* Sporophila digiacomoi *in an article by López-Lanús that is being distributed electronically as a pdf. The article is an "Adenda"* [Addendum] *to a forthcoming field guide to the birds of Argentina."* For further understanding go to the source: Claramount and Areta (2016), and S. Claramount in (*op. cit.*).

On this grossly false and unscrupulous basis, Claramount and Areta (2016) are responsible for the fact that the name *digiacomoi* is not available except as a synonymy of *Sporophila iberaensis*, contrary to the fact that the book and its respective addendum have existed in print since late 2015 via delivery/print-on-demand. After Claramount and Areta (2016) I donated a copy to the Louisiana State University Museum of Natural Science library, directly to J.V. Remsen (Jr) who duly thanked me for the copy: "*YES, it arrived today! Congratulations on a fantastic book that will be very useful. Thanks also for signing the book. Gratefully.*" (J.V. Remsen *in lit.* 5/OCT/2016). The publication date of the book is December 2015. Although Remsen (2022) [and collaborators] is the *alma mater* of the *South American Classification Committee* (hereafter SACC), the "confusion" was not cleared up and the information in Proposal 715 of Claramount and Areta (2016) was never corrected.

At the same time as this publication, *Sporophila digiacomoi* [orum] (López-Lanús, 2015b) was included in the Official Register of Zoological Nomenclature (Zoobank). Furthermore, all existing publications on whether *digiacomoi* is an available name later than *iberaensis* are left without argument, especially when Di Giacomo and Kopuchian (2016) did not substantiate their species and promised to describe it later (which never happened). Finally: the following publications and opinions *in lit*. fall into a fallacy on the basis of Claramunt and Areta (2016): J.V. Remsen, J.F. Pacheco and F.G. Stiles in Claramunt and Areta (2016), Di Giacomo and Kopuchian (2016), Areta *et al.* (2016), Schulenberg (2016), Gallupi-Selich *et al.* (2018), Turbek *et al.* (2019), Pearman and Areta (2020), Browne *et al.* (2021), Turbek *et al.* (2021), Roesler and Monteleone (2021), M. Pearman, I. Roesler, D. Gallegos, F. Gorleri, J. Quillén Vidoz, J. Baigorria, D. Monteleone, E. Jordan and S. Imberti in Roesler and Monteleone (2021), Remsen *et al.* (2022), V. de Q. Piacentini (*in lit.*). D. Monteleone, E. Jordan and S. Imberti in Roesler y Monteleone (2021), Remsen *et al.* (2022), V. de Q.

Piacentini (*in lit.*). The name *digiacomoi* was always available, as it could be uploaded to Zoobank at a later date and whoever wants can do it. Moreover, the validity of the proof of the taxon and its year of publication prevail regardless of the year of registration in Zoobank. It is striking in the history of ornithology to see a group of unsuspecting ornithologists being swindled and who do not react; the example of the citations given suffices. Presumably, these authors unwittingly incurred the fallacy of Claramunt and Areta (2016).

Note: According to Areta *et al.* (2016) it can be inferred that the Ibera Seedeater has another name available on the basis of *Sporophila melanops*. These authors conclude that *S. melanops* is not a good species [there is a single male specimen collected in 1823, nominated as such almost fifty years later (Pelzeln, 1871)]. They speculated that it could be attributed to the Dark-throated Seedeater *S. ruficollis* with a melanic cap; or alternatively to what we now know to be the Ibera Seedeater with that melanic cap; they then add that they cannot be sure that it is not a hybrid between *S. ruficollis* and a Yellow-bellied Seedeater *S. nigricollis*. In the event that it is not a hybrid, they mentioned that ruficollis should be called *S. plumbeiceps*, and the other form (the one from Iberá) *S. ruficollis*, which is why Pearman and Areta (2020) only included the Ibera Seedeater as Sporophila sp. (see: M. Pearman in Roesler 2021). I would like to add as the descriptor of *S. digiacomoorum* that since there are no recordings of individuals with '*melanops*' plumage, as it has not been found again after intensive searches (see Areta *et al.* 2016), frankly it is very risky to attribute another available name to *S. digiacomoorum* on the basis of: 1) an atypical plumage obtained in 1823, 2) erroneously nominated as a new species, 3) being a specimen of an unknown species with a melanic cap (or any other colour as so often happens in seedeaters), and 4) not being able to find new specimens to analyse acoustically and/or molecularly. It will have to be demonstrated. But in any case, it is illogical to proceed with this, as it is not an expected plumage as a colour morph of *S. digiacomoorum*, and any aberrant colour could even correspond to other taxa of the seedeater group; for example, the atypical plumages of the Black-bellied Seedeater *S. melanogaster*, which Areta *et al.* (2016) did not take into account, should be included in the colouration possibilities. In other words, the spectrum of possibilities is very wide and after Areta *et al.* (2016), it is striking, with *S. digiacomoorum* being a true species with *terra typica* (López-Lanús 2015b; 'Di Giacomo and Kopuchian 2016'; Turbek *et al*. 2021), that no black-capped plumage was ever recorded during six years of intensive studies. It is objectively surprising that this hypothesis continues (see Pearman and Areta: 2020, M. Pearman in Roesler: 2021).

Note 2: The correct ending of the specific name '*orum*' (*digiacomoorum* = of the) and not '*i*' (= 'of' for a single male person: *digiacomoi*) as given in López-Lanús (2015b), is based on Schulenberg (2016).

Sicalis holmbergi (López-Lanús, 2017): **VENTANIA YELLOW-FINCH**.

This species was described by López-Lanús (2017). The book as such follows the same parameters as the first edition and print-on-demand (see ISBN 2017/2022: 978-987-42-3178-9). *Sicalis holmbergi* as a taxon does not appear in global bird lists (Clements *et al.* 2021 [eBird/Birds of the World-CLO], Gill *et al.* 2022 [IOC: International Ornithological Committee], HBW and BLI 2021 [Tobias *et al.*/BirdLife Interntional]). For example, it is not mentioned by Pearman and Areta (2020), although it is striking that they cited López-Lanús (2015b) en relación al Capuchino Iberá, and López-Lanús (2015a) in relation to the validity of *Bubo magellanicus* (Tucúquere). Neither did they mention the Lenga Cinclodes (see below); therefore this ambivalence makes this publication unobjective in taxonomic terms. The basis for the absence of the form *holmbergi* in these publications may have to do with Pearman and Areta (2017). These authors titled Proposal 748 to the SACC "Recognise *Sicalis holmbergi* as a new species", advising "not to recognise it "after their justifications. Considering that the previous proposal (Proposal 715 for *Sporophila digiacomoorum*) generated an induced result using a false basis, it is not surprising that it happened again, but this time with a different strategy. The lightness with which the authors treat the López-Lanús (2017) study is so extreme that it is necessary to respond to them point by point, not to them, as they do not seem serious, but to the *voters* who gave their opinion with the same lightness, to such an extent that it attracts attention. Each argument used by Pearman and Areta (2017) is presented below, contrasted with my respective opinion as counterpart, in most cases with existing information in the same publication that describes the species. Phrases in italics in inverted commas correspond to Pearman and Areta (*op. cit.*).

1) *"Putative new species / on the basis of a single specimen / and photographs of sound-recorded individuals"* : They condition the recipient from the outset with "putative" species. The number of recordings is significantly higher than the number of photographs (all deposited in MACN). The main rationale of López-Lanús (2017) is based on bioacoustic analysis with a generous N, photography is tangential. It was decided not to make a collection of skins due to the precautionary principle in terms of conservation.

2) *"Unconventional and complicated* [description] *as the description of the* Sporophila *in SACC Proposal 715 (Claramunt and Areta: 2016)"* : A value judgement that is subjective. Moreover, it is not conditioning. The information is well presented even if it does not suit the taste of the reviewer. Although the ideal is to publish an exemplary description, the minimum data for the interpretation of the study are amply presented. The citation of Claramunt and Areta (2016) is not understandable, since it is not conditioning and López-Lanús (2015b) exceeded expectations for the description of this taxon (later reconfirmed as a species).

3) *"The same author chose to publish this description in a privately published bird guide"* : It is not conditioning. The comment is not understood. There is no private publication but that of the author's Editorial Audiornis, which has published other books, including by different authors. The description of S. holmbergi can be found as an addendum in a book freely available to the public (ISBN 978-987-42-3178-9, print on demand). The author chose this mode of publication because it is in his right to do so, especially to counteract the pernicious rise of the concept of "anonymous reviewers" in indexed journals.

4) *"No reference to an entry in ZooBank of the species name"* : Simultaneously with this publication, *Sicalis holmbergi* (López-Lanús, 2017) was included in the Official Register of Zoological Nomenclature (Zoobank). Whether it was entered at that time or today is not a condition for the identity of *S. holmbergi*. The description and validity of a species is independent of whether or not it has an entry in ZooBank, in fact the Ibera Seedeater alluded to above is registered in ZooBank with a publication without real content that promises a description in the future (Di Giacomo and Kopuchian 2016) which they never did (beyond ignoring the formal description of López-Lanús 2015b).

5) *"We assess the description by its merits regardless of whether it formally fulfills the requirements of the International Code of Zoological Nomenclature"* : If it does not meet the requirements of the ICZN, the "deference" of still analysing it is not understandable. Rather, it seems that the authors are making sure that the species is annuled as was done in Claramunt and Areta (2016) with *S. digiacomoorum*, since they mentioned it, and by association Krabbe (2020) with *Cinclodes lopezlanusorum* (below).

6) *"The morphological diagnosis is problematic and insufficient, and does not suffice to justify full species status"* : Totally in agreement, not in terms of the problematic (?) but in terms of the insufficiency, in particular as it is a cryptic taxon in its morphological aspect as a twin species of *Sicalis auriventris*. The morphological diagnosis presented in López-Lanús (2017) is secondary due to the mentioned condition (a practically cryptic species), but the justification for the species status is based on the behaviour (bioacoustic analysis, display), radically differentiated from *S. auriventris*. This differentiation is so radical that a subspecies status has been ruled out.

7) *"First, it does not provide any character to diagnose the holotype of* holmbergi *from other* Sicalis *in fresh*

plumage" : Same as the previous comment, the character to diagnose it is precisely that, its non-differentiation in plumage, except some morphometric differences that due to the low N of the sample (only one specimen) does not assure a diagnosis on that account. The diagnosis is obtained from behaviour (in particular vocalisations), with a high N to ensure a constant in this new taxon (bioacoustics, display, habitat use, endemism condition, conspicuous acoustic differentiation while the entire population of *S. auriventris* presents no differences in vocalisation in the Andes in its entire cline).

8) *"But notably fails to do so from* auriventris*"* : A rhetorical comment, given that López-Lanús (2017) emphasises its similarity to *S. auriventris* and its differentiation (at least in the adult male) is not notable but non-existent. Again: the diagnosis for this cryptic species is emphasised in the behaviour (vocalisations).

9) *"At this point, it must be said that this specimen was identified as* S. auriventris *in Areta et al. (2012)"* : Since the publication of López-Lanús (2017), the error of Areta *et al.* (2012) has been corrected, which those authors were not obliged to notice as the species was published *a posteriori*.

10) *"Who also mentioned a work in progress by Pearman and Chiappe in this regard"* : The same comment made above applies, because it is not conditional; even less so if that study does not exist to date. Still, it should not necessarily be considered as that species by argument of authority, moreover, it is not necessary to ask for permission to prove otherwise. Or what is worse: if it was done, it would not succeed "because it has already been published or promised to be published but has not yet seen the light of day".

11) *"Moreover, we do not see any difference in terms of plumage, shape, and size between the male holotype and fresh-plumaged* auriventris*"* : Idem comment above, being a cryptic species, the questioning is diluted. Clarification: in their comment they refer to the holotype of *S. holmbergi* as "*auriventris*", so the observation is inductive. Again, they are twin species, of the cryptic type. The diagnosis is made by behavioural parameters such as the analysis of their vocalisations (crucial and diagnostic) and nuptial display, etc. All this information can be found in López-Lanús (2017), well presented and clear.

12*) "Second, the type specimen is claimed to have a slightly longer bill (exposed culmen 0.3 mm longer than the largest* auriventris*), a wider mandible (0.7 mm wider than the largest* auriventris*), and a longer tarsus (1.1 mm than the largest* auriventris*), but is indistinguishable in other measurements from* auriventris*. These tiny differences might well be the result of measurement error or just indicate slightly larger measurements in some variables of an otherwise normal individual of* auriventris*"* : This is correct, but it has an explanation already remedied in López-Lanús (2017). These are the measurements and proportions based on a single specimen of *S. holmbergi* compared to the *S. auriventris* sample consulted. The authors (Pearman and Areta 2017) mentioned a possible measurement error (happily anyone who wants to can go and consult the specimen in the MLP), or else an overlap in measurements due to small individual differences. The latter is entirely feasible, but they do not mention precisely a main point emphasised in López-Lanús (2017: p. 482): that the diagnosis between the two species is "inversely proportional: large bill (and apparently short wing) in *S. holmbergi* versus shorter bill (but longer wing) in *S. auriventris*. In the rest of the *Sicalis* all the values obtained are below the holotype measurements, even in the larger range". Therefore, any minimal disparity in measurements does not change the fact that *S. holmbergi* is a true species, since the diagnosis is by far and above all "through" its vocalisation (as emphasised in López-Lanús 2017). On the other hand, it would be very good if Pearman and Areta (2017) would refer to the source of these (their) measurements.

13) *"Third, the author acknowledges that birds in typical [worn] breeding plumage ('typical plumage breeding male', also called 'deep yellow') are indistinguishable from* S. auriventris*"* : Apparently, it is only at this point that Pearman and Areta (2017) understand the scope of the study. Of course they are indistinguishable. It is the characteristic of *S. holmbergi*. This is why the publication of López-Lanús (2017) seems *"unconventional and complicated"*, as referred to above, as it is necessary to demonstrate that the diagnosis lies in behavioural data, as classic as its vocalisation. If the study ended up with many pages, it is precisely with

criticism in mind, such as that of the Proposals of the SACC where Pearman and Areta give their opinion, which when the time comes, in the end they act as "peer reviewers"; but in the instance of a review for publication, it would be rejected with the same lightness with which Pearman and Areta (2017) argued in their "Proposal".

14) *"Fourth, the long primary projection that is so typical of* S. auriventris *is clearly visible in the type specimen and in all pictures of live individuals in which wing shape can be assessed, and the straight to very slightly curved culmen typical of* S. auriventris *is also evident in all known pictures of the Sierra de la Ventana birds"* : It does not change the above, and again... it would be very convenient if they would cite the source of this assertion which we all know, but there is no publication on the subject. Further studies are needed to quantify what they observe, measurements? demonstration? any publication? For this purpose, photos are of no use. In parallel, it does not change the diagnosis as a species, with the emphasis on behaviour (vocalisation); for it is known that there are differences in the shape of the culmen, for whatever reason, in other *Sicalis*.

15) *"The diagnosis based on vocalizations is unconvincing and does not suffice to justify full species status"* : On this point, it is only necessary to refer to López-Lanús (2017) to verify the fallacy of the comment. To reply to this runs the risk of "getting wrapped up" in the intention of these authors by their method of inductive detraction. However I answer "convincingly".

16) *"First, the spectrograms are of such poor quality that one is left wondering where the similarities and differences lie in terms of vocalizations"* : Idem above. Please refer to López-Lanús (2017) and check the fallacy.

17) *"The statement that 'The notes in the introduction of S.* holmbergi *are diagnostic due to their simple structure, mainly composed of pointed inverted Vs*" *is erroneous"* : On the contrary, it is the best of the diagnostic features of the species. I refer to López-Lanús (2017) and then to Fig. A1-3 (opp. pag.) for proof of the fallacy or, unfortunately ruse, of Pearman and Areta (2017) to invalidate my work. It is a disgrace that they lend themselves to asserting that the inverted pointed V "is wrong". I invite these authors to formally publish what they say and refute it without compromising their careers. Figure A shows how they distort the inverted sharp Vs by stretching them to almost a horizontal note. Normally nobody studies an audio-spectrogram in such detail on a "note" scale, and in milliseconds of duration, when one wants to interpret the "phrase" (or first stanza of a chant-like vocalisation, to give an example). It can be done, of course, but when comparing one taxon with another, the aim is to show the "perspective" of the audio, and to show the differences as they appear in any audio spectrogram as soon as it is opened in the programme. This can be checked by simply entering the acoustic specimens that they themselves expose in their Figure, treated here in Fig. A by copying theirs, with a recording of my authorship, and the other of I. Areta, deposited respectively in the MLNS and Xeno-Canto (MLNS-517968 / XC343658). See Fig. A and you will see the gimmick used, which at first sight in that publication appears to be the final blow to my demonstration.

18) *"As can be seen in the spectrogram that we made using different acoustic parameters (Figura A1)"* : In view of those spectrograms to which Pearman and Areta (2017) referred (already analysed in the previous point), if they were not only trying to mislead the general public but also the illustrious voters in the SACC proposal (= A. Jaramillo, K.J. Zimmer, F.G. Stiles, J.F. Pacheco, M.B. Robbins, S. Claramunt, D. Cadena), it remains to question their ability to be in the position of evaluators or reviewers in any scientific environment. If there was no malice in their exposition, at least they engaged in a conceptual barbarity without precedent. They used millisecond-long notes to compare with other notes, which is valid but out of context. In López-Lanús (2017) for that type of vocalisation, the exposed notes are compared within the context of a phrase (= the song) in the same way that xeno-canto (2022) or e-Bird (2022) show their sonograms.

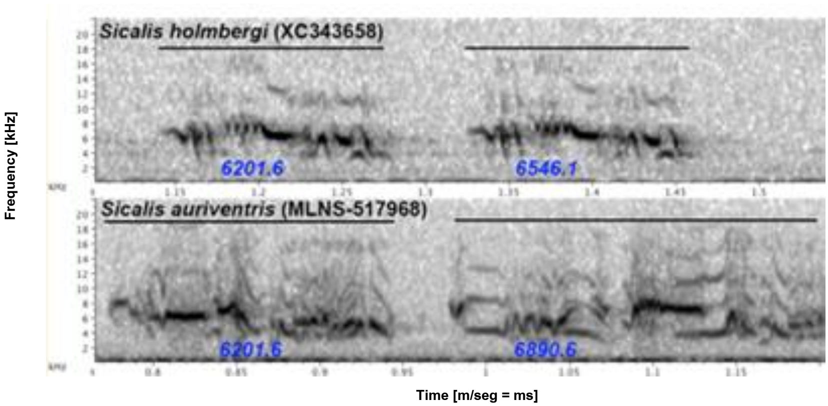

Figure A1. Representative sonograms of *Sicalis holmbergi* and *S. auriventris* presented in Pearman and Areta (2017) with which they plot the non-differentiation between the vocalizations of both taxa, denying that *S. holmbergi* presents notes with inverted Vs shape (contra López-Lanús 2017), supposedly assimilating to the vocalization to *S. auriventris*.

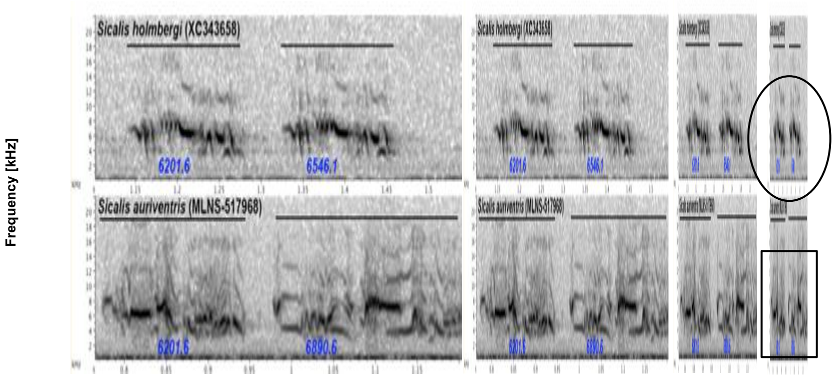

Figure A2. The same sonograms presented in Pearman and Areta (2017) taken sequentially to the time parameter used by López-Lanús (2017), not on note scale but on phrase scale (as is normally stipulated for this type of analysis). Note that the last image on the right fully matches the shape of the notes presented in Fig. A3. Without the unusual gimmick of changing the timing parameters in the sonogram ("stretching the notes"): the discourse of Pearman and Areta (2017: Proposal SACC 448) is unfounded.

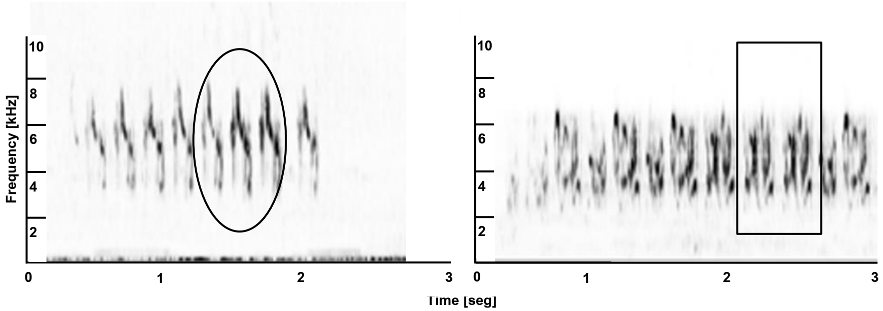

Figure A3. Sample of representative sonograms published in López-Lanús (2017): *Sicalis holmbergi* (left) and *S. auriventris* (right). They show the typical song of each species as a whole (= phrase), for a duration of 3 seconds (not one or two notes for milliseconds). Note how the selected notes in the sonogram on the left (*S. holmbergi*) coincide with the mark in Fig. A2 (above).

19) *"This spectrogram* [theirs] *shows that each supposed note is made up by complex notes in quick succession, so that there is not a simple structure and that there are no inverted Vs."* : False, it is not visible at that scale. Clarified above. Precisely, shown as they did in Fig. A1 of their publication, they look like the same notes. But see Fig. A3 and note the inverted pointed Vs, as shown in López-Lanús (2017). In the example of Pearman and Areta (2017) with their Fig. A1, showing two notes to that scale does not make sense; furthermore, by announcing that "each supposed note is made up of complex notes in rapid succession", by using the word "supposed" they only make the public doubt whether what they show is one note or a set of notes. But in a study on this scale it is "one note". It cannot be compared by leaving out the "perspective". That instance is for analysing a note (or "presumed note") if necessary, but in the example they apply, one would expect even intraspecific, individual differences and variations in notes within the same phrase.

20) *"The aspect of introductory notes as characterized by the author arises as the product of a poor choice of spectrogram parameters that do not \ show the fine structure of the sound"* : Such a statement crowns the lack of seriousness of these authors, because of the above. The analogy is like deciding to carry out a census of beaches on a maritime coast by analysing the number of edges of each grain of sand. Rather than a conceptual problem, Pearman and Areta (2017) are disrespecting the scientific community with their elaboration of Fig. A1 (see Fig. A2). The choice of parameter, i.e., displaying frequency ranges in Herz and duration in seconds in the audiospectrogram, is the same for all examples presented in both species. Therefore the comment is absolutely extraordinary.

21) *"These vocalizations defy easy characterizations. Note that in the sample showing the 'notes' of* S. auriventris*, their complex nature is also evident. "* : Idem. All these notes have a complex nature. Thus they can all be characterised as demonstrated in López-Lanús (2017). The notes of *S. holmbergi* are observed as inverted pointed Vs, in *S. auriventris* this pattern does not occur: independently of the complex nature of each note in the whole vocal repertoire of both taxa and other *Sicalis* as well. The differences are also noticeable aurally. The sonograms presented here are merely representative samples in order to be able to interpret the "difficult" characterisations and to visualise with quantifiable results where the vocalisation that characterises *Sicalis holmbergi* stands out from *S. auriventris*. On a note scale, as these authors have done, the futility of their presentation is demonstrated.

22) *"Second, the author simply states that* auriventris *has a conservative song pattern throughout its distribution, but the songs of* auriventris *are very complex (coming in simple and complex versions, as in all* Sicalis*)"* : False. López-Lanús (2017) refers to a certain type of song. Of course, "all" *Sicalis* have variations from simple to complex in each of their "types" of vocalisations, but they maintain identifiable intraspecific patterns, a situation that can be verified once an analysis has been carried out as a whole, with a high N, as observed in Areta *et al.* (2012) and in López-Lanús (2017).

23) *"And vary geographically in some features"* : Correct, but throughout its distribution in the Andes from north to south *Sicalis auriventris* has well-defined and stable vocalizations throughout its cline as demonstrated in López-Lanús (2017). Just visit the collection of MLNS or Xeno-Canto sounds to analyze it and check it in the same way that it was done in my study.

24) *"Despite this,* auriventris *seems to share the general song structure with* holmbergi: *in both cases a rather monotonous series of metallic tinkling notes can be followed by a variable number of series of different notes and sometimes a long chatter"* : On the contrary, the general structure of the song is not shared. If it "seems" to them, they should verify it with a study which, to date, does not exist except that of López-Lanús (2017). In this study there is no such "seems", but its differentiation has been demonstrated. On the other hand, I show that *S. holmbergi* has a song that is more similar to *S. uropygialis* than to *S. auriventris*; but Pearman and Areta (2017) did not mention this fact. The tinkling monotonous series is described and analyzed in

López-Lanús (2017), but they only repeat what had been stated and they eluded the similarity observed with *S. uropygialis*.

25) *"Third, the quantitative acoustic measurements lack any rigor and are presumably the product of data taken from the visual inspection of spectrograms"* : An unusual comment. Go to López-Lanús (2017) and see the scientific rigour with which each type of vocalisation is treated in both *S. holmbergi* and the rest of the species. The frequency distributions are described on comparable scales between species, and the frequency and duration ranges are quantified.

26) *"For example, the mention of a higher pitch in the vocalizations of* holmbergi *does not hold in a simple analysis: peak frequencies in the random spectrograms here shown are astonishingly identical in one note of* holmbergi *and* auriventris*, and higher in a second note of* auriventris*"* : In fact, what is astonishing is that the result obtained in López-Lanús (2017) is misinterpreted, as hundreds of phrases in different territories with populations of *S. auriventris* and a similar number of recordings were analysed not just two notes. The higher "pitch" in *S. holmbergi* is perceived not because of the frequency range which, as Pearman and Areta (2017) pointed out, are "astonishingly identical", but because aurally it is noticeable that they sing with a higher pitch. This is because **the greater amplitude ('volume') in the frequency distribution of each note occurs at higher ('upper') frequencies in *S. holmbergi* than in *S. auriventris***. Once again, Pearman and Areta (2017) misrepresented the information given in López-Lanús (2017) and with the example of Fig. A1 that they presented, they believed they were being forceful in their analysis. Precisely, "these vocalisations defy easy characterisation", and the isolated example of a higher note in *auriventris* is once again mentioned out of context.

27) *"Finally, geographic variation in vocalizations of Oscines deserves careful analysis, and does not automatically imply species status for any such vocally different population"* : I have already commented above about the reference to the lack of strong or perceptible variation in *S. auriventris* throughout its north-south range in the Andean "band", in contrast to the striking differentiation of the vocalizations of *S. holmbergi* in its endemic range in Ventania. Apart from the "songs", it should be remembered that there is much less variation in the Oscines between the populations of a particular species when analysing alarm or contact calls, especially the short and emphatic ones consisting of "simple" notes. It is striking that Pearman and Areta (2017) did not mention the relevant fact that the entire population of *S. auriventris* has this type of call with an ascending frequency distribution (in its entirety), while in *S. holmbergi* it is definitely descending. This characteristic is not a good ally for the auditory diagnosis of the species (in the field), but in the face of an audiospectrogram, once again, it is conclusive.

28) *"Indeed, work in* Sporophila *has shown considerable geographic variation in vocalizations without advocating species status for any of those vocally distinctive and allopatric populations"* : There is no need to analyse regiolects in *S. auriventris* and *S. holmbergi*, as in the Andes only one type of vocalization is recorded (see López-Lanús 2017), whereas in Ventania the vocalizations of *S. holmbergi* are radically different to the point that they are distantly similar to *S. uropygialis* and not to *S. auriventris* (see López-Lanús 2017), and above all, because they have a diagnostic vocalisation.

29) In Fig. A1 *"Spectrograms showing the complex structure of each "note" (delimited by horizontal lines) in* S. holmbergi *and* S. auriventris*. Peak frequency measurements for each note (in blue) and the spectrograms were made using the same spectrogram parameters and the same sampling rate in both recordings"* : The maximum frequency measurements in blue taken from "two notes per species" (see Fig. A1) only indicate the frequency range of those notes, not the largest amplitude (volume) of the note in a certain area of its frequency distribution. At this scale it is ridiculous to show this situation, as it is interpreted correctly by analysing whole phrases, not two isolated notes "stretched" to show their individual complexity, to the detriment of the analysis of the song as a whole.

30) The *"Playback experiments (n=6 experimental subjects) were conducted without rigor. It is not clear whether each individual was subjected to multiple stimuli or not, and there is no indication as to which playback stimuli were used"* : The robustness of this type of experiment is subjective, which is why the experiment is mentioned tangentially even though it was carried out. The methodology applied was the standard one, with territorial proclamation songs, first of *auriventris*, then of *holmbergi*.

31) *"Local stimuli seem to have always been tried last, among many other shortcomings. Despite this, the author interprets his results as indicating that individuals from Sierra de la Ventana respond to vocalizations of this same population but ignore those of* S. auriventris*,* S. uropygialis*,* S. olivascens *and S. lebruni. Lack of methodological rigor suggests that these results must be taken with skepticism"* : This is not the situation of López-Lanús (2017), but the preference of Pearman and Areta (2017) on how to carry out the experiment, so I make the same comment that I made above. As I already stated playback experiments can be misinterpreted by the observer, and even by the birds! Therefore, the results can be subjective.

32) *"The presumed display flight of* S. holmbergi *is so poorly described that it is not clear exactly what this display entails. Singing in flight, which species such as* S. olivascens *and* S. auriventris *do undertake (contra López-Lanús 2017), is different from having a parachuting display such as those of* S. luteola *and* S. citrina*. The author provides no data on where and, if indeed, when he studied* auriventris *in the field. "* : When opining "contra López-Lanús 2017" it would be correct if they had used "contra Areta et al. 2012", as my information is based on what they described. Regarding the nuptial flight that I described, this seems to be a case of denial of reality on the part of Pearman and Areta (2017). Otherwise, they would not describe it as "presumed". The description of the nuptial flight is as described, and it was not intended to overcome the astonishment of those authors. The flight is as explained, and on the basis of Areta *et al.* (2012) only *S. luteola* and *S. citrina* perform this type of flight. The information they claim to have has not been published up to that date, otherwise they would have mentioned it as my omission.

33) *"Until more behavioral data are presented, we are unable to confirm that this flight display in* S. holmbergi *is indeed diagnostic"* : The data on flight behaviour are well detailed as reported by López-Lanús (2017), in any case data on the type of flight in *S. auriventris* should be published now that it is known (according to what they state) that they do some type of flight while vocalising, not the "parachute" type. The type of flight in *S. holmbergi* is complex, similar to that of *S. luteola*, "parachute" type; the denial of Pearman and Areta (2017) seems to lie in not accepting the novelty published in López-Lanús (2017).

34) *"Finally, the author does not mention the existence of populations of three other Andean/Patagonian bird species in the Sierra de la Ventana:* Catamenia analis*,* Asthenes modesta*, and* Agriornis montanus*. Although further study of these populations is warranted, the examination of specimens by Pearman and Chiappe (unpublished MS) leaves no doubt that these are all populations either directly assignable to the Andean subspecies or at most subspecifically differentiated taxa"* : Extraordinary. It is not conditioning. The comment is surprising.

35) *"In this context, the lack of diagnostic features of* S. holmbergi *from the Andean/Patagonian* S. auriventris *lends further support to our view that* holmbergi *is no more than an isolated population of* auriventris*"* : In fact it seems to be the case that the data of López-Lanús (2017) do not support the diametrically opposite position of Pearman and Areta (2017). Having analysed their opinion, not a demonstration (even with the ruse of Fig. A1), the effort and determination to ignore and misrepresent the results obtained by López-Lanús (*op. cit.*) is surprising.

36) *"This description vividly illustrates the problems for bird taxonomy when a manuscript is not published in a peer-reviewed journal"* : It is precisely because of the "tone" of everything commented above that the species was published in the way it was. Otherwise, under the influence of Pearman and Areta as experts on the birds of the region - as demonstrated by way of example - *S. holmbergi* would most likely never have seen

the light of day. It is disconcerting that in a Proposal with the prestige that the "SACC" receives, such openly hilarious statements can be made. It is because of attitudes and falsehoods such as those expressed in the SACC, with actors who make up the list of reviewers of prestigious ornithological journals, that much of the knowledge publishable by other colleagues is lost or, in short, publications such as mine, necessarily "without peer review", occur.

37) *"For the large number of reasons stated above we recommend a NO vote"* : The authors involve the voters in a multi-point misrepresentation and achieve their recommendation. If "*It all comes together in the SACC*", as already quoted above, at least this note demonstrates for the second time how such an organisation can present data.

Five years after the publication of *Sicalis holmbergi* as a new species in López-Lanús (2017), it can be understood from the analysis of the 37 points above why the taxon does not appear in the bird lists, as it "does not exist" since Pearman and Areta (2017). As they are well established authorities as bird experts at the regional scale, the scope their influence can have, in this case, pernicious, is demonstrated.

Cinclodes lopezlanusorum (López-Lanús, 2019): **LENGA CINCLODES**

"*How could they tell you anything in Inat?* [iNaturalist]. *That Lenga Cinclodes is a figment of the imagination. It is impossible to identify it. Either we ignore it and pretend it never happened (ideal) or if we consider that it might exist all Cinclodes in winter are sp. (which would be crazy)*" (I. Roesler, a local leader in response to a question asked in a well-known forum, on Twitter: 11/MAY2022). Personally, I can say: it would be the same thing that happens with the Ticking Doradito *Pseudocolopteryx citreola* remaining as *Pseudocolopteryx* sp, as it does not vocalize in winter within the territory of the Warbling Doradito *P. flaviventris* (which does not vocalize in that season either). And it's not delusional. If it is presumed that *Cinclodes lopezlanusorum* (Lenga Cinclodes) could be the species recorded outside the lenga in winter in an inter-Andean valley in Patagonia, the option *Cinclodes cf. lopezlanusorum* should be included, and if the data comes from the north in full migration, that of *Cinclodes cf. fuscus*. Those that are doubtful would be *Cinclodes* sp.; once again, "it is not crazy". What seems to be the case is that the ornithological community is not prepared to live with doubts in the case of gaps in knowledge. It happens with the albatrosses *Thalassarche cauta* and *T. steadi* for example, sometimes they cannot be identified. There are so few cases of cryptic (or temporarily cryptic) species that this should not be a cause for concern.

To conclude with the taxonomic status of three recently described species, *Cinclodes lopezlanusorum* was published in López-Lanús (2019) as a clear protest to: 1) the somewhat perfidious influence in peer review by ornithological journals (which is not counteracted by the request for abstention of appointed reviewers; even less by the system of anonymity); and 2) the strong abstention in academia from fully approving a new taxon if it does not pass through the sieve of molecular analysis, when behavioural analysis is equally effective (already demonstrated with Ibera Seedeater: López-Lanús 2015b versus Turbek *et al.* 2021); in addition to being the first detector of new taxa (par excellence: via bioacoustic analysis). As in previous cases, this species is described in an addendum at the end of the Audiornis guide to the birds of Argentina, ISBN 978-987-783-666-0 (ISBN 2019/2022), and also in the English language version (978-987-86-4257-4). As expected in the SACC Proposal saga, it was recommended (and voted again with one hundred percent of the votes) not to be considered a true species (see Krabbe 2020). Each argument used by Krabbe (2020) is set out below, contrasted with my respective counterpart, in most cases with existing information in the same publication that describes the species. Phrases in italics in inverted commas correspond to Krabbe's questioning (*op. cit.*).

1) *"Undoubtedly inspired by the splitting of* Upucerthia saturatior *from* Upucerthia dumetaria *(Areta and Pearman 2009), López-Lanús (2019) wrote a paper suggesting that* Cinclodes fuscus *is composed…"* : This situation is tangential and did not inspire any species. *C. lopezlanurorum* begins as an autonomous study because it was discovered as an exclusive inhabitant of the Andean-Patagonian forest, only in lenga (*Nothofagus pumilio*) forests. The situation posed by Krabbe (2020) is number 17, in last place of the 17 points of analysis of information obtained in the context of eight years of study and fieldwork (López-Lanús 2019: p.481). On the other hand, it is not a "suggestion" but I announce that it is: a true species.

2) *" …of two parapatric, sibling species, one, which he described and named* Cinclodes lopezlanusorum*, exclusively inhabiting* Nothofagus *forest, and the other,* Cinclodes fuscus*, exclusively inhabiting open areas of both Patagonian steppe and the high Andes. Unlike the case of the earthcreeper, however, these two putative species are so sibling that they cannot be told apart genetically, vocally, or morphologically"*: He induces the reader before starting the "case" treatment with the word putative. It is NOT exclusive to *Nothofagus* forest but to *N. pumilio* forest; otherwise it can be interpreted as inhabiting all forests with *Nothofagus* species (which is very different as will be seen later). It is correct that they are so closely related that they cannot be separated morphologically or by plumage, but the rest is incorrect and contrary to what is stated in López-Lanús (2019). For it is specifically mentioned that **they cannot be separated genetically "with the standard procedure used" (which is not adequate, as is often the case in "borderline" cases such as this one)**. Nor is he correct when he states that they cannot be differentiated vocally; on the contrary, **the vocalisations can be easily separated by means of audio-spectrographic analysis** (although not aurally). **Furthermore, it has an exclusive song** (diagnostic including aurally) **accompanied by an exclusive behaviour** (nuptial flight in the high vegetation of the lenga forest), which is absent in *C. fuscus*.

3) *"It is stated that apart from habitat they differ from each other in some vocalizations, display flight, migration, and time of breeding, and that none of them occurs in the ecotone between their respective habitats, but the sample sizes are much too small to substantiate any of this"* : False. The sample size is more than satisfactory as can be seen in López-Lanús (2019), conveniently written for criticisms of this type, where the article does not skimp on data, tables and audio-spectrograms of both taxa for the various types of vocalisations that are most common. It is striking that he makes the comment when it is contrary to the truth. For other data, such as migration, breeding anachronism and ecotone records, the sample size is satisfactory and constant for all cases with repeated visits to the study areas (limited to a few days of breeding activity before nesting because both species soon become mostly stealthy and silent).

4) "*The elaborate descriptions of a 'display flight' are particularly disturbing"* : The comment is unusual, bordering on a value judgment. See below.

5) *"Like* Upucerthia *species (Areta and Pearman 2009),* Cinclodes *species are not known to have ritualized display flights at all, but may sing from perch or in flight alike"* : Precisely, this display flight is unique to *C. lopezlanusorum*. Not only does it perform it in a limited pre-breeding period and/or full breeding (nesting), but the song that accompanies this behaviour is unique and diagnostic, differentiated from the rest of its vocal spectrum, which, moreover, has never been observed in *C. fuscus* despite the attempt to obtain the observation of some even a similar behaviour, since as Krabbe (2020) rightly comments, it is not characteristic of any known *Cinclodes*. N. Krabbe denies one of the main diagnostic elements that characterize this forest species.

6) "¿Why does the author not simply conclude that like Andean and Chilean flickers *Colaptes rupicola* and *C. pitius*, the Buff-winged Cinclodes will nest in any cavity, be it in a tree, an embankment, or between rocks?" : Because the sample size is sufficient [contrary to the supposed deficit of López-Lanús (2019) in this respect]

to confirm that *C. lopezlanusorum* nests only in in large specimens of *Nothofagus pumilio* in positive hydric gradients, not using the same supply of cavities in forests of other *Nothofagus* species and other genera of the Araucanian forest, nor cavities in bare ground, or ravines with bare stony soil, or between rocks in any of the "peripheral" sites surrounding that type of forest (López-Lanús 2019), even rejecting them with less nest density towards the periphery of those forests (Ojeda 2016). And also because: *C. fuscus* does not nest in ecotone areas, nor does it nest in the northern Patagonian steppe as a passage population to the south, whereas the population of *Cinclodes lopezlanusorum* does (anachronistic reproduction); and in southern Patagonia both populations do so simultaneously, with *C. lopezlanusorum* restricted to within forests of only *N. pumilio* (no others) in and *C. fuscus* to the open steppe, with no presence of *Cinclodes* of this group in ecotone areas, discarding ravines and rocks for nesting; being parapatric species without intergradation in habitat use (non-syntopic). Because in *C. lopezlanusorum* the migration path through the passes from Chile to Argentina from the west in northern Patagonia, it starts its reproduction in lenga forests early, unlike what happens with *C. fuscus*. *Cinclodes fuscus* returns to the steppe from the northern plains but does not breed in northern Patagonia until it reaches southern Patagonia. *Cinclodes lopezlanusorum* reproduces markedly earlier in northern Patagonia, a situation typical of two disjunct populations.

7) *Why simply... "the ecotone where none of them was found was not investigated thoroughly enough or simply lacked suitable nesting sites?"* : The ecotone was explored, studied and even 'experienced' in the same way as the Andean-Patagonian forest and also the steppe (*C. lopezlanusorum* only being found in a certain lenga environment, and *C. fuscus* in the middle of the steppe environment).

8) *Why simply the...* "*vocal difference just reflects the range of variation within a single species?"* : Before answering why, it is striking that Krabbe (2020) now refers to a difference in the vocalisations of *C. lopezlanusorum* when he previously commented that they could not be differentiated vocally. The answer is because within the north-south Patagonian forest gradient the Lenga (*C. lopezlanurorum*) population only presents vocalisations with reverberation and therefore **fewer notes per second** and, on the other hand, the steppe population only presents vocalisations without reverberation (in the absence of a cathedral-like green dome) in its breeding area (on the few days that they vocalise openly, otherwise they are very quiet) presenting **more notes per second** [In addition *C. lopezlanusorum* has a nuptial display song, which Krabbe (2020) apparently denies and therefore does not allude to it, allowing the question].

9) *"*Cinclodes fuscus *gives a number of different vocalizations, so when dividing the recordings into the 20(!) different vocal types suggested, there are only few examples of each. To find two that are directly comparable is only possible with the most common vocalizations, and there is a good chance some of the vocal types would be missing from any set of recordings with a similar size as that presented (38 of* C. lopezlanusorum*, 31 of* C. fuscus*)"* : Apart from the number of vocal types in *C. lopezlanusorum* and *C. fuscus*, all analogous except for the sexual display in *C. lopezlanusorum*, they present fewer notes per second in *C. lopezlanusorum* (due to the habitat they frequent and the green cathedral-like dome effect) than in *C. fuscus* (with a higher number of notes per second due to the absence of obstacles that generate reverberation), whatever the type of recording selected for each taxon, especially the trills. The examples of quantifiable trills add up to hundreds of examples for both taxa (= not a few). This fact, isolated from any of the 17 other situations that confirm *C. lopezlanusorum* as a true species, would not be sufficient; but within that set of differences (differentiated migration, anachronistic nesting, etc., etc.), the vocal particularity in the number of notes per second in each taxon is one of the most revealing and compelling results.

10) *"I compared 146 recordings of* C. fuscus*, 22 of* C. oustaleti*, 10 of* C. olrogi*, and 59 of* C. albiventris*, and found that song varies considerably in composition between individuals, sometimes even within a bout of song by a single individual. In fact, except for the most common call, I did not find any two recordings of different individuals to be as identical as recordings of most other suboscines are. The same has been reported to hold*

true for Upucerthia *(Areta and Pearman 2009)"* : The comment developed above applies exactly. It does not matter whether or not the vocalisations are the same in a song type, even in the same individual in a long sequence. What matters for the vocal diagnosis of *C. lopezlanusorum* or *C. fuscus* is the number of notes emitted per second, a differentiation that in *C. lopezlanusorum* has to do with reverberation due to the habitat it occupies during breeding (fewer notes per second due to the phenomenon of reverberation), a limited period in which it performs a song of territorial proclamation. Reverberation is an 'artefact' and not part of the song of the species: see López-Lanús (2019), but not analysed in Krabbe (2020). That this phenomenon generates a diagnostic differentiation in the number of notes per second in the vocalisation of a species is unprecedented.

11) *"It is extremely tedious to work through the 34 pages of the paper. It almost seems as if the author has deliberately both convoluted and stretched the relevant data into a nearly unreadable form and then used a rare font (Agency FB) that makes one feel like being in a labyrinth"* : It is not conditioning. The 34 pages are for the critics, otherwise it would take up less space. It is preferable to have a lot of information, rather than the criticism that there is a lack of information. The typeface used in the first Audiornis editions (Agency FB) is not a condition for not being able to discern the conclusiveness of *C. lopezlanusorum*: it should not be interpreted that a species is not a species because of the typeface used by the author to describe it. On the contrary, denying a reality supported by data from the outset (long or not) is what would give rise to the labyrinth commented on by Krabbe (2020).

12) *"It appears that the approach is to come up with possible ways of making data fit the initial theory (that two species are involved), rather than focusing on the likeliness of alternative explanations"* : The alternative explanation is that this is a forced case of *C. fuscus* within *C. fuscus*; i.e., a "subspecies", but with a population within a population with a "magical" plasticity summarized in 17 analysis points that separates them. That situation is what would be "conforming to the alternative theory". But the support of *C. lopezlanusorum* as a valid taxon on the basis of 17 diagnostic points (López-Lanús 2019) dilutes the search for an alternative explanation precisely because it is linear and forceful.

13) *"That the two are genetically identical in Cytochrome C oxidase subunit 1 does not deter López-Lanús, who does not even bother to give details on methods and procedure of the genetic work"* : I cannot be deterred, and I appreciate the protest, in the face of a more eloquent one. For example: most of the *Sporophila* of the 'capuchinos' group were considered a polymorphic species by Campagna *et al.* (2009) with that kind of molecular methodology. Much later, the Ibera Seedeater, as a full species based on vocalisations (López-Lanús 2015b), was found to be valid (Turbek *et al*. 2021): differentiating genes associated with plumage (validating against all hypotheses that it is a true species). This fact indicates that the validity of this species was already demonstrated in my 2015 paper, relativising the forcefulness of many molecular studies. That is why I do not "bother to give details about the methods and procedure of the genetic work". Standardised molecular analysis for many twin/cryptic species is dispensable.

14) *"He simply considers it a case of recent speciation and compares it to the* Pseudocolopteryx flaviventris/citreola case *(see SACC proposal 420), omitting the fact that those two species, although genetically very similar (Jordan 2018), differ drastically in vocalizations and do not respond to each other's songs"* : The vocalisations of *C. lopezlanusorum* also differ drastically from *C. fuscus*. The application of audiospectrogram analysis shows that this is eloquent, even if aurally the difference does not seem drastic to us; the comment is entirely subjective from the typical *Homo sapiens* perspective. Apparently, for the two *Cinclodes* discussed here, the difference is drastic, or the reverberation (without being an echo) "dulls" the message and therefore it is slower (refer to López-Lanús 2017). In addition

to the other 16 reference points that support *C. lopezlanusorum* as a true species, there is a drastically distinctive vocalisation, the nuptial display song. As a cryptic species, its genetic and vocalisation similarity should not draw attention; the comparison with *Pseudocolopteryx flaviventris/citreola* is valid.

15) *"It is noteworthy that of the 25 days of field work mentioned, 16 were spent in Santa Cruz (Calafate to PN Los Glaciares), where Imberti (2005) had found* C. fuscus *in both forest and open areas, whereas López-Lanús during three consecutive years there only found it in open areas. This casts doubt on the significance of observed absence"* : Precisely, after three years, I managed to find the elusive *C. lopezlanusorum* that inhabits the Patagonian Andean forest in Los Glaciares N.P., following the clues of Imberti (2005, S. Imberti *in lit.*). I tried to find them breeding for three years until I located them in a specific site with suitable lenga. I always found *C. fuscus* vocalising (nesting) in the steppe, even close to the limits of the national park. Knowing that it is only possible to catch the vocalisation for a few days during breeding (hence the limited visits to the study sites, which do not add up to more than 25 days), thanks to S. Imberti (in lit.) and many other collaborators I finally managed to reach the Lenga Cinclodes at Río Guanacos, which, in his work on Birds of Los Glaciares N.P., he described "in forests" without considering the possibility of the two species. For this reason, he does not comment on this, but only describes the habitat. There is "no doubt on the significance of observed absence", I found both species in Los Glaciares N.P. in two well-differentiated environments (non-syntopic species).

16) *"On top of that, the mere idea of an ecotone with suitable nest sites not being occupied by any of the two strikes me as unlikely"* : This is precisely one of the points that make the diagnosis of both species. It is not improbable, but real.

17) *"Like the two other new species of birds described in appendices to previous releases of Guía Audiornis (Sicalis holmbergi [= S. auriventris; see SACC proposal 748], and Sporophila digiacomoi [= S. iberaensis; see SACC proposal 715]), the description of Cinclodes lopezlanusorum has not been through a critical reviewer process"* : As is stated in the present paper on the taxonomic status of three newly described species (precisely the three spp. that Proposals 715, 748 and now 846 have dismissed), such unserious reviewers and critics should not intervene in descriptions of species that stand on their own despite so many ill-founded objections, most of which can be refuted by a cursory reading of the original publication.

18) *"Two anonymous reviewers are thanked in the acknowledgments section, but as the author and the editor are one and the same, there is no guarantee that the reviewers' recommendations were followed. In fact, I strongly suspect that they were not"* : This comment contains a value judgement. As an author I have my own judgement and right to express it in public. *Serpophaga griseicapilla*, the species described by R. Straneck had a peer reviewer but the journal in which he published it is not a specialist in zoology but in zootechnics and veterinary science (Straneck 1993): in any case his description passed the SACC sieve. If R. Straneck wanted to be able to publish his study without the hindrance of criticism (pers. comm.) on that occasion the formal reviewers could not contribute much, but it was published anyway. In Argentina a periodical such as Nótulas Faunísticas has its reviewers but they are anonymous. At the Audiornis publishing house I apply the same criteria. These positions are perhaps the result of abuse in other well-known journals/journals. All in all, all the buts in these three Proposals, which I have overcome with my objective response, would be enough to dispense with such reviewers retroactively.

19) *"I can only recommend that you vote NO to this proposal"*: And so do: J.V. Remsen, J.I. Areta (already a classic, I refer to serious reading of López-Lanús 2019 to dismiss his remarks), E. Bonaccorso, A. Jaramillo, S. Claramunt, K.J. Zimmer, M.K. Robbins and J.F. Pacheco.

Discussion

A trend in the comments of committees, such as the SACC in South America or CARO in Argentina, is the need for an author to have credibility through peer review in order to achieve legitimacy in their publication. Therefore my paper on *Bubo* (López-Lanús 2015a) published in the journal El Hornero, which enjoys peer review assigned by its editor, was not accepted within the SACC. *Bubo virginianus nigriceps* was not elevated to *Bubo nigriceps* as a species having been demonstrated; nor was *Bubo virginianus magellanicus* elevated to *Bubo magellanicus* having also been demonstrated. The fact that a paper has a peer reviewer assigned by an external editor does not always guarantee the achievement of an objective by the author; no matter how well he or she has substantiated his or her work. Robbins and Nyári (2014) publishing in *The Wilson Journal of Ornithology* could not get the SACC to approve the existence of 9 species among the 19 subspecies of *Cistothorus platensis*. Cabanne *et al.* (2019) publishing in *Molecular Phylogenetics and Evolution* failed to convince critics that "appearances can be deceiving" by splitting *Syndactyla cabanisi* from *S. rufosuperciliata* as a species. And what to say when hybridisation is faster than mutation in the emergence of new species, as published in *Science* by Lamichhaney *et al.* (2017) with "Big Bird", the new Galapagos *Geospiza* unnamed by the Grant couple (its discoverers) for fear that inbreeding will make them extinct? Criticism is influential.... And so the cases multiply.

The question should be asked whether it is healthy for ornithology to have committees that decide everything about everyone else, based on a handful of "experts" who are only human. Especially if in these commitees the voters are made to understand the opposite of what the author under discussion has published, and the voters themselves can corroborate it, and not having to be the one in a publication such as the present thats calls their attention. On the other hand, comments such as *"The emblematic case is Krabbe's proposal on Cinclodes lopezlanusorum, whose hilarious argumentation is worthy of a comedy show, needless to say that he absolutely and indeclinably rejects it"* (D. Gallegos Luque in Roesler 2021), *"It looks like a vedette fight in Carlos Paz: jealousy, betrayal, fake publications, and other miseries"* (D. Gallegos in Roesler and Monteleone 2021); or *"Acting in haste with this species has already practically claimed the ornithological career of B. López Lanús and the confidence in him"* (M. Pearman in Roesler and Monteleone 2021) could be indicating that an Argentine committee of ornithological records serves as a mechanism to discredit other colleagues. If these statements were true, the Ibera Seedeater would not be described, because it would never have been published as they were imposing on me, and I reserve the right to do so as its discoverer. Five years went by before a molecular study that ratifies it today. The good news is that this molecular study was not essential, and there is nothing to indicate that the essence of observation and the gift of classical ornithology will be lost. N. Krabbe (whom I hold in high esteem) should apologise for reading too lightly into my work, as should many others; and with regard to "Carlos Paz", if it is about me, I make it clear that I do not accept bullying and no one should take the candy out of anyone's hand. Maybe we should lose confidence in those who represent CARO or SACC. If this continues, it seems more like a group of self-convened friends talking informally, gathering by necessity "some isolated dissenting vote" to legitimise them in the eyes of the ornithological community, with no control on what they publish, and the tone in which they do it.

These are the words of La Grotteria (in Roesler and Monteleone 2021): *"We are still waiting for the 'SACC proposal badly needed'...* [for *Sporophila iberaensis*], *leaving a striking contrast where the SACC only took a couple of months to invalidate* S. digiacomoi*, but the assessment of* S. iberaensis *has not yet arrived, after more than 5 years".* Apparently my same detractors did not notice that the promised publication by Di

Giacomo and Kopuchian never arrived. López-Lanús (2015b) is available, now that it is known that it always existed; it is a good time to ratify it (for those sceptics) with the confirmation of Turbek *et al.* (2021). *Sporophila digiacomoorum* has been proven to be a true species from the beginning, validating once again that bioacoustic analyses are preponderant in terms of taxonomy. This is why La Grotteria added (*op. cit.*): *"in any case it is necessary to wait for information to see if they refute its taxonomic status or claim its name"*. In this regard, if one speaks of refutation, similar studies should be published to support that refutation, not applying a set of loose phrases readable in ten minutes in each committee. As for *"...or see if they claim its name"*. Well, obviously a claim, such as the present one, would have to be made. I can only add that the "circus" created in the committees by their malfunctioning is ultimately the result of a political attitude on the part of their members: to be in control. Otherwise, the unusual and fallacious degree to which the discussions on these three species have reached cannot be understood. Since all of them were added gradually to the Audiornis guide in the course of its editions since 2015 (totalling 3500 printed books and thousands of PDF copies), I owed my readers this explanation.

Conclusion

In this paper the detractions from the taxonomic status of three recently described Passeriformes were analysed. The analysis was carried out on the basis of citable sources, consisting of opinions, or even biased and/or misrepresented information published by ornithologists representing renowned institutions in the scientific community, or their own careers as scientists. Regardless of the prevailing standards of rigour in scientific methodology and the institutions that created and defend them, it is to be hoped that the community that respects and follows them will not deprive themselves of one essential element: the truth in the presentation of data in whatever source of publication. Especially if that source is not formal and peer-reviewed as it is so often claimed to be. The omission of part of the data published by me, today exposed and clarified by way of a "reponse"; or the falsification of the same (which is even more alarming), seems to represent the very highest intention on the part of those colleagues who have expressed so much opinion. Their discourse "sounds changed" in terms of the veracity and accuracy of the available published data, instead of being objective and clear in favour of the welfare of the scientific community. As already seen, the criticisms by the actors mentioned in the main body of this paper made of my methodology and/or the results obtained do not differ from other studies considered valid, even by those same actors. Such criticisms appear as verdicts in committees of universities or institutions, and/or in groups in social media with a large number of followers. In these, opinions are expressed and the committees also vote among a selection of expert ornithologists, who do not demonstrate the supposed "shortcomings" of the taxonomic results obtained by any author by means of a formal publication, or with a clear, comparable, respectable N. It seems to be, just a feeling, that these institutions and those who represent them use their position of influence to impose their ideology on the scientific method ("argument of authority" in short, and generous "sophism") without presenting, as already said, an equally comparable, convincing publication, and that the discourse they use goes from being an opinion to a fact. I hope I am wrong and that this whole situation is an unhappy coincidence. Otherwise, I would stop to analyze the underlying perniciousness of groups of people who are admirable for their scientific rigor, but with a profuse hint of personal interest that clouds their objectivity, "above" prestigious scientific institutions, or of scientific dissemination. In this case, the “fraternity“ (referring to a group of self-convened individuals) imposes itself *a priori* on "objective impartiality". Some scientific institutions, or those in the social media (but who ultimately represent them), should be held accountable for the opinions that they so blithely express to the academic public, but also to students in training, or to those who make up the "citizen science" component: so permeable to the following of scientific heroes (often derived in a "triumphant childish admiration"). On the other hand, if this practice (surprisingly volatile committee opinions) continues without being supported by studies that analyse any publication with the same meticulousness, this criticism should not fall into profanity (disqualifying epithets), lack of ethics (falsifying data) and loss of professional restraint (not accepting different opinions and other orthodox resources as methods of demonstration). In short, not to fall into "political bias" due to a prevailing line of thought, as seems to be happening, which occurs when personal convenience and control over the work of other colleagues by any person or legal entity (institution) prevails. In short, a few representatives of science should not impose this or that truth about public knowledge on the rest of the ornithological community.

So, at some point it had to happen... *Who of all must be the one who dares to put the bell on the cat?* This is the last stanza of a very short but forceful poem by Lope de Vega (1562-1635). It alludes to the impediment of being able to carry out something because of its threat and danger; for which reason volunteers are called upon to make a decision that is profitable for all. I have not only responded to the criticism made by so many colleagues (already cited in this paper) over seven years in which I have been silent (2015-2022). I have also analysed all sorts of scathing scientific opinions, with no hard scientific proof, and in the case that there was only to reconfirm a previous result of my authorship (i.e., the case of *Sporophila digiacomoorum* in relation to the paper of Turbek *et al.* 2021). As a corollary, more surprising than anything else, is the extent to which I have reached the desks of these institutions and the authorities that represent them "even if my work is not citable". My call for composure and the need to put the bell on the cat is thanks to this and the "dust" that I may have raised. Any "similar sound" to the analogy used, now warns us. For ironically, cats would not do as much harm to ornithology as many ornithologists do to this discipline. That is why I had to describe those three species "outside the sometimes flawed system", and a new one in this very book ratifying my position: ornithology has no owner, and its discoveries should not be subject to any group of people, or their plunder.

I suggest taking the narrative published in committees “with a grain of salt” and let ornithology continue to make its way in publications, not opinions.

The mice gathered together
in order to escape from the cat;
and after a long while
of disputes and opinions,
they said that they would do well
by putting a bell on him,
because walking with it on,
they could better escape from him.

But one said to the Roman Senate,
after talking culturally for a while:

Of all of you, who will be the one
that dares to put
that bell on the cat?

Lope de Vega (1562-1635)
Adaptation

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